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**FINAL TECHNICAL REPORT
for
NASA GRANT NAGW-2451**

"DISCOVERY PROGRAM"

Principal Investigator: Professor Joseph Veverka

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**CENTER FOR RADIOPHYSICS AND SPACE RESEARCH
CORNELL UNIVERSITY
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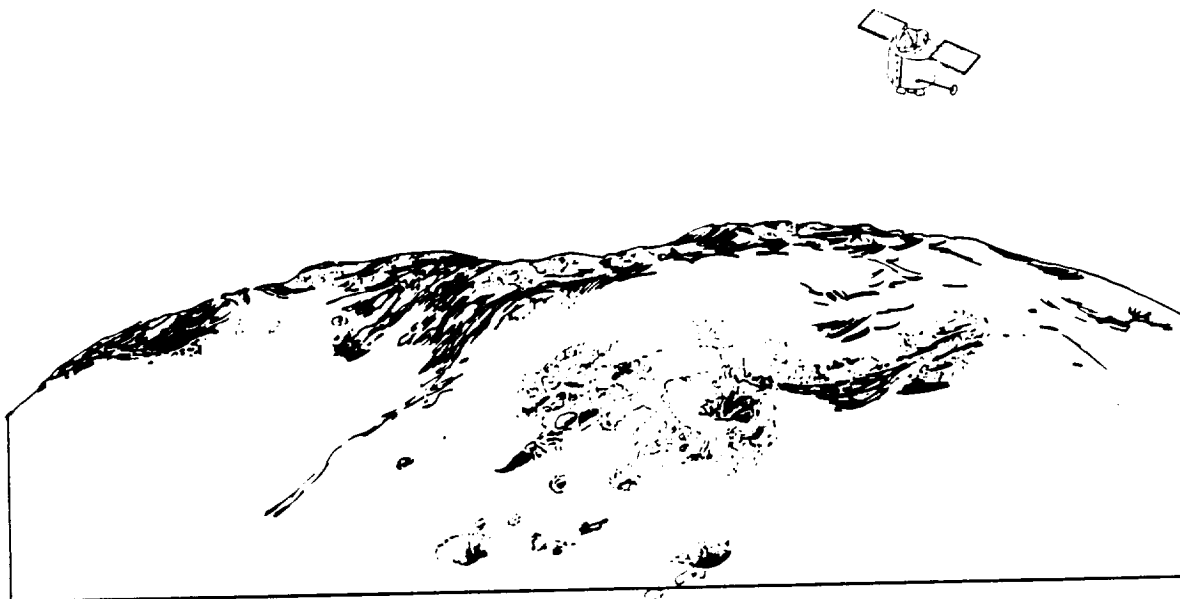
"DISCOVERY PROGRAM"

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The work carried out under this grant consisted of two parallel studies aimed at defining candidate missions for the initiation of the Discovery Program being considered by NASA's Solar System Exploration Division. The main study considered a Discovery-class mission to a Near-Earth Asteroid (NEA); the companion study considered a small telescope in Earth-orbit dedicated to UV studies of solar system bodies. The results of these studies are summarized in the two reports which are attached (Appendix 1 and Appendix 2). Copies of these were transmitted to NASA in 1992.

DISCOVERY:

Near-Earth Asteroid Rendezvous (NEAR)



Report of the
Discovery Science Working Group

Executive Summary
October 1991

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EXECUTIVE SUMMARY

MAJOR RECOMMENDATIONS

- The first mission of the Discovery Program should be a rendezvous with a Near-Earth asteroid.
- The target asteroid should be chosen on the basis of minimizing mission complexity and cost.
- A suitable target is asteroid 1943 Anteros. This mission requires an FY94 or FY95 new start to meet the May 1997 launch opportunity.
- On the average, one suitable rendezvous target per year is available.
- The rendezvous payload should consist of three instruments in addition to Radio Science: an imager, an IR spectrometer/mapper, and a gamma-ray spectrometer.
- Follow-on missions should include flybys as the optimal way of investigating the known diversity of Near-Earth Objects (asteroids and comets).
- The SWG urges NASA HQ to proceed expeditiously in implementing the first Discovery mission. An essential element of this process is to initiate Phase A activities during FY92/93. Both mission/spacecraft study support and instrument development money are needed.

WHAT IS DISCOVERY?

The Discovery Program is a proposed new initiative by NASA's Solar System Exploration Division for low-cost planetary missions. Exclusive of launch and mission operations costs, each mission is to cost no more than \$75M in any given year, and \$150M or less overall. Precursor studies have shown that a significant number of scientifically important and exciting, focussed planetary investigations can be carried out within this fiscal envelope.

Due to its low cost, such a program when implemented will provide more frequent opportunities for participation than is the case with the more expensive and less frequent missions which currently make up the Solar System Exploration Program. The Discovery Program will also provide the possibility for quick responses to emerging scientific opportunities, and rapid project turnaround. All of these factors will lead to a broadened participation by planetary scientists, and facilitate cooperative efforts with other space agencies.

THE FIRST MISSION

The guidelines given the Science Working Group in defining the first Discovery Mission were that the first mission should be one of exploration to a relatively easy target. Accordingly, the Working Group focussed its efforts on the study of a mission to a Near-Earth Asteroid (NEA), and in particular on a mission which involves a rendezvous with, and going into orbit about, the target body.

Near-Earth Asteroids include some of the most accessible space targets, and have never before been explored by spacecraft. As detailed below, a rendezvous with an asteroid will provide answers to fundamental scientific questions, most particularly those dealing with the elemental composition of the asteroid, questions which cannot be addressed by flybys. Although flybys of several mainbelt asteroids are currently planned as parts of other missions (Galileo, CRAF, and Cassini), a Near-Earth Asteroid Rendezvous (NEAR) mission carried under Discovery will be not only the first ever investigation of any Near-Earth Asteroid, but the first rendezvous ever with any asteroid. As such, it will provide data that will not be available from any of the flybys.

The population of small objects whose orbits take them into the neighborhood of Earth (Near-Earth Objects, or NEOs) includes comets in addition to asteroids. The Discovery SWG recommends that the first Discovery mission target be an asteroid, rather than a comet, for several reasons. First, a rendezvous with an asteroid is much less hazardous (and hence less difficult and costly). Second, spacecraft propellant requirements are less for asteroids. Finally, an elaborate rendezvous with an active comet is the focus of the CRAF mission, whereas no other rendezvous missions with asteroids are planned.

IMPORTANCE OF NEAs AND SCIENCE OBJECTIVES

As of July 1, 1991, the orbits of about two hundred NEAs were tabulated. The smallest known NEA is about 6 meters in diameter (1991BA), while the largest is some 40 km in length (443 Eros). Most NEAs are relatively small, of the order of 1 km in diameter. It is estimated that there could be 1000 NEAs larger than 1 km in diameter. Populations in the 1m-100m size range are still largely unknown. Many of the NEAs currently known belong to spectral class S. A continuing debate focusses on whether S-asteroids are the source of ordinary chondrites, the most common type of meteorites that fall to Earth.

A reconnaissance of these objects is an important element in the Solar System Exploration Program because:

- (1) A systematic scientific exploration of the NEOs will yield details of the processes that have governed the formation and evolution of our solar system as evidenced by cometary nuclei, asteroids, and meteorites. Specifically, the NEOs may preserve clues to the nature of planetesimals

from which terrestrial planets formed. They certainly are the source of most meteorites that strike the Earth. They are a very diverse class of objects, including primitive and evolved bodies. Some of the NEAs may be dormant or extinct comet nuclei.

- (2) NEOs represent the primary source population of relatively large objects that strike the Earth, and which have influenced the evolution of the Earth's atmosphere and biosphere.
- (3) NEOs may represent a potential source of raw materials for the future utilization and exploitation of space.
- (4) Their low gravity, combined with the possibility of abundant H₂O, make them realistic candidates for future sites to develop the techniques of human deep space exploration.

The importance of asteroids in the overall study of the solar system and the scientific objectives of asteroid missions have been reviewed by several committees during the past decade, including COMPLEX. These objectives were reviewed by the Discovery Working Group and adopted as the basis of its deliberations. The Discovery SWG also made use of a previous NASA study of a rendezvous mission to a Near-Earth Asteroid (the 1986 NEAR Report; JPL 86-7).

Asteroid science objectives established by COMPLEX are:

- (1) To determine the composition and bulk density.
- (2) To investigate the surface morphology, including evidence for endogenic and exogenic processes and evidence concerning interiors of precursor bodies.
- (3) To determine internal properties, including states of magnetization.

These goals can be translated into the following three primary measurement objectives:

- (1) To determine the gross physical properties of the object, including size, shape, configuration, volume, mass, density, and spin state.
- (2) To measure the surface composition of the object. Elemental abundances and abundance ratios are required at a precision of 0.5% for all elements present at greater than 1% of the mass of crust. In addition, spatially-resolved near-infrared measurements of surface mineralogy are required to link compositional properties to exogenic and endogenic processes evident from surface morphology.

- (3) To investigate surface morphology through comprehensive imaging under a variety of lighting conditions at scales of 1/1000 the object's radius (or better).

Secondary objectives include:

- (1) To determine regolith properties and texture through imaging to centimeter scales.
- (2) To measure interactions with the solar wind and search for possible intrinsic magnetism.
- (3) To search for evidence of current activity as indicated by dust or gas in the vicinity of the target.
- (4) To investigate the internal mass distribution through measurements of the asteroid's gravity field and the time-variation of its spin state.

The Discovery Science Working Group concurs with earlier studies that there are primary objectives that cannot be addressed by flybys. Not only is it the case that one gets more and better data from a rendezvous mission (because one can spend an arbitrarily long time studying the target), but data on elemental composition of the surface cannot be obtained from fast flybys with available instrumentation. Currently, the only reliable method of gathering such data remotely is through the use of gamma-ray and x-ray spectroscopy; both methods require integration times that cannot be achieved during flybys.

The minimum science payload described below satisfies all of the primary science objectives, and most of the secondary ones.

MINIMUM RECOMMENDED PAYLOAD

To achieve the major science objectives listed for the Discovery NEAR mission, the SWG recommends a minimum payload of three instruments: an imager, an infrared spectrometer/mapper, and a gamma-ray spectrometer in addition to Radio Science. The gamma-ray spectrometer is the only means of remotely determining the elemental composition of the target to the level of accuracy required. Imaging provides the primary means of determining the physical, geological, and morphological characteristics. The infrared spectrometer is the primary means of determining mineralogical composition in a spatial context. All three instruments work together to infer the heterogeneity of the surface. The Radio Science experiment will determine the asteroid's mass, which in combination with the volume determined by imaging, will place significant constraints on the asteroid's interior through the mean density.

The SWG stresses that the above is a minimal payload, determined in the spirit of achieving the key science objectives within the constraints of the Discovery mission. Additional instruments would certainly yield valuable science. But it is the Working Group's conviction that a payload of more than 3-4 instruments will strain the resources of the first Discovery mission.

In the case of three of the investigations—imaging, IR spectroscopy/mapping, and radio science—suitable instrument designs exist or are being developed. In the case of the gamma-ray investigation, a new instrument tailored to the needs of the Discovery NEAR will have to be developed. One possibility is a body-mounted, uncooled, sodium-iodide detector instrument modelled after the successful Apollo design. The Mars Observer instrument, with its associated boom and coolers, is too cumbersome for a Discovery-class mission.

RENDEZVOUS AND ORBITAL OPERATIONS

Studies during the past year confirm that given an imaging system on the payload, there should be no difficulty in targeting the spacecraft to the vicinity of a small asteroid and matching velocities with, or going into orbit about, the target. Given that one of the key instruments, the gamma-ray spectrometer, works best in as low an orbit as possible, certainly within 1-2 radii of the surface, and preferably closer, studies were undertaken to determine the feasibility of operating a Discovery-class spacecraft safely at such small distances from an irregular body. These studies suggest that once the mass, size, and shape of the target have been ascertained with precision, operations at 1-2 radii of the surface will be feasible without undue burden on mission operations. Closer orbits might require frequent maneuvers and could lead to considerable mission complexity. The SWG recommends that the goal of the first Discovery mission should be to operate at or beyond about 1-2 radii of the surface, and that excursions closer-in be reserved for a possible extended mission.

While it is in some cases possible to fly by one target on the way to a rendezvous with another, the SWG recommends that in general such flybys be included only if they do not significantly add to the cost, risk, or complexity of the primary rendezvous mission.

TARGET SELECTION

From spectral measurements, it is evident that the near-earth population contains a wide diversity of objects, in terms of composition, evolutionary history, and provenance. The SWG identified important specific questions pertaining to particular types of asteroid, but felt that overall the choice of a target for the first mission should be dictated by mission feasibility. The SWG concluded that any NEA (provided that its diameter is at least 500 meters) is a suitable target for the first mission. Other things being equal, one should launch to the easiest target to maximize mission payload/capabilities. Asteroid "type," size, and likely provenance are not considered essential selection criteria for the first mission.

However, given the wide diversity of known NEOs and the different fundamental scientific issues concerning each type, the SWG felt that the first Discovery mission must be considered as one element of a broader investigation program of Near-Earth comets and asteroids. Any follow-on Discovery NEAR mission should go to a different type of target. Full use should be made of flybys to address the known diversity of NEOs and in particular to study the energetically hard-to-reach component of the population that has eccentric and inclined orbits, and which is most likely to consist of comets in various degrees of activity (active, dormant, extinct).

MISSION OPPORTUNITIES

Currently the orbits of some two hundred NEAs are known. Suitable targets for Discovery missions are those NEAs whose orbits are relatively well-known and which can be reached within the available launch capability. Additional constraints involve mission duration and communication distances. Available launch capability excludes rendezvous missions to NEAs with markedly eccentric and/or inclined orbits.

Mission and spacecraft studies were undertaken in support of the SWG activity, both at the Applied Physics Laboratory of Johns Hopkins University and at the Jet Propulsion Laboratory. The JPL study was supported by a number of contractor studies. The mission is to be constrained to the costs defined for the Discovery Program (total cost less than \$150M, a maximum spending within any year of \$75M), and to the allotted launch vehicle capability (up to that of a Delta 7925). This capability makes it possible to rendezvous with targets requiring a ΔV of 5.5 km/sec or less from a 200-km circular Earth orbit. The total mission duration is to be constrained to three years, with at most two years trip time to the target, and one year devoted to the rendezvous study.

Both the APL and JPL studies concluded that a rendezvous mission to a Near-Earth Asteroid carrying at least the minimum number of instruments recommended by the SWG is possible within the constraints of the Discovery Program. These studies were assessed by a special "Discovery Program Cost and Management Team" led by James S. Martin, which summarized its findings in July 1991 as follows:

"The Discovery Program Cost and Management Team concludes that solar system science projects can be implemented for less than \$150M (FY92\$) by focussing science goals, using mature instrument and spacecraft technology, and constraining the development schedule to less than three years. Such projects will add significant benefits to NASA's overall planetary science exploration program."

THE RECOMMENDED MISSION

SWG recommends that NASA seek a new start in FY94 or FY95 for the Discovery NEAR. An FY94 or FY95 new start will enable a launch in May 1997 to 1943 Anteros, an S-type asteroid some 1.8 km in diameter. The ΔV required, 5.35 km/sec, is within the Delta capability and the flight time to rendezvous in this case is only 415 days. Backup opportunities to other NEAs are available, for example, a launch in December of 1997 to asteroid 1982 XB, a 0.5 km, S-type. Ample opportunities for Discovery-class rendezvous missions exist; on the average, one can expect at least one opportunity per year.

In this context, the Discovery SWG urges NASA to expand its support of telescopic studies of NEOs. Such studies will lead not only to the discovery of further potential targets, but most importantly, to a better definition of the orbits and physical characteristics of these bodies.

GENERAL CONSIDERATIONS

It is clear that in order to achieve the goals of the Discovery Program, simplicity and efficiency must be the watchwords. The spacecraft, science payload, and mission designs must be kept as simple as possible. There must be sharp focus on key objectives. Everything must be streamlined, and there must be a willingness to accept reasonable risks. **The SWG urges NASA to adopt a new, more streamlined approach to the management of Discovery.** Discovery cannot be run in the same mode as recent larger missions, where often process has gotten in the way of product, and micromanagement has flourished at all levels. Cumbersome, redundant reporting so common in most NASA programs should be eliminated on Discovery.

The SWG urges NASA to make maximum use of industrial and institutional expertise in carrying out the Discovery Program. There is also a strong sense among the members of the SWG that investigations on Discovery NEAR should be carried out by small PI-led teams rather than as "Facility" investigations which often involve redundant levels of management.

SUMMARY OF RECOMMENDATIONS

The SWG recommends that NASA's Discovery Program be initiated with a rendezvous mission to a Near-Earth Asteroid (NEA). The group's analysis indicates that such a mission, carried out within the cost/complexity/risk envelope of the Discovery Program, can achieve significant science objectives in the study of these important bodies which cannot be addressed by other means.

The SWG finds that the major science objectives of such a mission can be achieved with the radio system and a minimum payload of three instruments: an imager, an IR spectrometer/mapper, and a gamma-ray spectrometer.

Of these instruments, the gamma-ray spectrometer requires further development beyond the Apollo prototype design. The SWG urges that funds be made available as soon as possible to carry out the detailed design of a gamma-ray spectrometer suitable for the Discovery NEAR mission. A conceptual design of an instrument based on an uncooled NaI detector which can achieve the measurement objectives desired was carried out by a subgroup of the SWG and is outlined in the Main Report.

To keep within the guidelines of the Discovery Program, it is mandatory that the payload for the first mission not be overselected. Specifically, no instruments of novel design whose proper performance is premised on "inventions" can be included in the payload. We also urge that the Science Teams responsible for the instruments be kept small (to limit cost) and see no advantage in operating these groups in anything but the "PI mode."

The SWG feels very strongly that the target for the first mission be selected largely on the basis of mission performance considerations. An NEA target should be chosen which does not strain any of the available resources. While the group had extended discussions concerning the relative science merits of studying various NEAs, the final consensus was that the important science goals of such a mission could be achieved no matter what the "spectral type" or the "diameter" of the target, provided that the object is at least 500 meters in diameter. Given the population of NEAs, the target of the first mission is likely to be an S-asteroid with a diameter of 2 km or less.

Our study indicates that suitable NEA targets are available on the average at the rate of about one per year. The discovery rate of suitable targets, and the accurate determination of their orbits needed for mission planning, would benefit from a modest increase in the support that NASA provides to telescopic observers through its Planetary Astronomy program.

Flybys on the way to a rendezvous are sometimes possible, but should be considered only if they do not add significantly to the cost, risk, or complexity of the primary rendezvous mission.

While comets form an important component of the Near Earth Object (NEO) population, the groups recommends against choosing an active comet as the target of the first Discovery mission. The reason is both practical and scientific. From a practical point of view, a rendezvous with an active comet is a much more hazardous (and hence expensive) endeavor than a rendezvous with an asteroid. From a scientific point of view, a rendezvous with an active comet is the major objective of the CRAF mission, whereas no rendezvous with any asteroid is currently planned outside the Discovery program.

The NEAR mission proposed to initiate Discovery should be considered in the context of a program to study the NEO population as well as asteroids and comets in general. A

reasonable plan would be to include a backup spacecraft as insurance against the failure of the primary. Given the successful launch and operation of the first spacecraft, the second could be sent to study a different Near-Earth asteroid. Given the diversity of objects among the NEAs this second mission would be very important in broadening our perspectives. However, bearing in mind that even two rendezvous missions cannot exhaust the diversity of objects to be studied, the SWG recommends that serious consideration be given to complementing the rendezvous recommended in this report with a series of less expensive flybys within the context of the Discovery program. Although such flybys cannot address the full range of important science objectives, taken in conjunction with one or two rendezvous, they do provide a realistic means of gathering some valuable data about the diverse objects (asteroids and comets) that make up the NEO population.

CONCLUSIONS

The SWG finds that a rendezvous mission with a Near-Earth asteroid is possible within the constraints of the Discovery Program, and recommends that NASA seek a new start in FY94 to take advantage of the May 1997 launch to 1943 Anteros.

As essential as the first mission is, a comprehensive study of Near-Earth asteroids and comets must involve a sequence of coordinated missions. We recommend that this coordinated program include both rendezvous (for detailed studies) and flybys (to investigate the full diversity of NEOs). While almost of necessity Discovery-class rendezvous missions should focus on asteroids, flybys should include comets.

APPENDIX 2

Report of Discovery SWG UV Sub-Group

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*based on a meeting held at the University of Colorado
April 18-19, 1991*

17 June 1991

EXECUTIVE SUMMARY

An Earth-orbiting ultraviolet planetary discovery mission can provide unique and complementary information on the Jovian system during the time of the Galileo mission's exploration of Jupiter's environment. This mission, which can be accomplished at low cost and on a short time-scale using state-of-the-art technology, will provide a global view of the interactions of the Jovian satellites, the magnetosphere and the atmosphere of the planet, and thus a context for the interpretation of the in situ measurements of particles and fields. Such information is strongly desired by the Galileo particles and fields investigators and cannot be provided, without serious impact on other mission objectives, by the Galileo ultraviolet instruments. The data to be obtained will also provide continuous monitoring of the time variable ultraviolet emissions which will be recorded locally, and in a snapshot mode, by the ultraviolet instruments on Galileo. The proposed single scientific instrument is a spectrographic imaging telescope in the extreme- and far-ultraviolet wavelength range (500–1750 Å) whose capabilities are complementary to those of both the Galileo UVS and those of the Hubble Space Telescope. Jupiter will be the only target to be observed over the course of 9 months during the Galileo mission (the solar elongation of Jupiter is greater than 45 degrees from 15 February to 22 November 1996 and again from 20 March to 28 December 1997 so there are two possible windows for our proposed mission) and the data to be obtained will continuously measure the evolution of the Io plasma torus and neutral corona of Io as well as the response of the Jovian atmosphere and aurora during this period. The data on Jupiter will be processed expeditiously and disseminated to the community including the Galileo investigators. Following the dedicated observations of Jupiter, observations of other solar system targets can be scheduled through a guest observer program.

With the focussed objectives stated above, the proposed payload can be built and flown at low cost and in a timely fashion so as to be in orbit by mid-1996. Reduced cost is achieved by combining a short schedule, available state-of-the-art technology and a lean management structure as well as through the adoption of an acceptable level of risk. No new development is foreseen. Our approach and implementation should provide a basis for the development of future Earth-orbiting planetary astronomy missions similarly dedicated to a single major scientific objective.

To meet the schedule based on the Galileo mission, it is imperative to begin a Phase A feasibility study no later than the first quarter of FY1992. We propose that this be done through the appointment of a Science Definition Team (SDT) by the Director of the Solar System Exploration Division. Such a team would be similar to the Instrument Definition Science Teams (IDSTs) that, in the past, have guided the development of JPL built facility instruments (e.g., imaging systems) for planetary missions, except that now the instrument to be defined includes the entire spacecraft and ground system. The study could be accomplished in six months as there exists a large amount of groundwork that has been done by members of the Discovery SWG (who would serve on the SDT) as a result of proposals submitted in response to the Small Explorer (SMEX) AO issued in 1988, as well as from work done on other current ultraviolet astronomy projects. Support for this study can come from funds originally earmarked for US support of the ESA/NASA/Germany Orbiting Planetary Telescope (OPT). It should be made clear that in no way is the proposed mission to be construed as an attempt to resurrect the panchromatic/multi-purpose OPT, as it represents a concept that is quite the opposite from that of OPT.

I. INTRODUCTION and RECOMMENDATIONS

Since the discovery, by the Voyager UVS, of the wealth of far- and extreme-ultraviolet emissions from both the plasma surrounding Jupiter and the upper atmosphere (polar and equatorial) of the planet, the importance of ultraviolet observations of the Jovian system from Earth-orbiting satellite observatories has been recognized as a means of monitoring the stability of the plasma as well as of investigating the detailed physics of the interaction between the plasma and the neutral atmosphere of Jupiter. Such observations also serve to constrain the theoretical models, developed since the Voyager encounters with Jupiter in 1979, based on both the UVS data and the in situ particles and fields measurements. Intensive modelling was particularly required for the UVS data, which were obtained at low spectral resolution (30 Å) and were limited in temporal coverage.

Most of the subsequent ultraviolet observations of the Jovian system have been made by investigators using the International Ultraviolet Explorer (IUE), launched as a joint NASA/ESA/UK project in 1978 and still functioning today. These have been very successful despite several limitations of IUE: small field-of-view; poor spatial resolution along the spectrograph slit; wavelength range > 1180 Å; long integration times required (comparable to Jupiter's rotation period) for the plasma torus; and the limited number of "windows" available to guest observers during a given year. Nevertheless, IUE has provided a measure of the long and short-term variability of both the plasma torus and the Jovian aurora since the Voyager epoch and has also been able to measure, for the first time, the atomic constituents of the extended atmosphere of Io.

Advances in ultraviolet technology have made possible much more sensitive instrumentation for the spectroscopy of faint extended astronomical sources. As an example, the Hopkins Ultraviolet Telescope (HUT), which flew on the Space Shuttle Columbia as part of the Astro-1 mission in December 1990, obtained high signal/noise ratio spectra of the Io torus at 3 Å resolution (10 times better than the Voyager UVS) with only 20 minutes of integration time. Similar resolution spectra were also obtained on the Jovian aurora and equatorial emissions. However, these are all "snapshots in time", as will be future observations of the Jovian system to be made by the Hubble Space Telescope (HST). Moreover, HST will also be limited to wavelengths longward of 1150 Å. HUT was an almost ideal instrument for investigating the Jovian system, as its spectral range extended from 415 to 1850 Å. The Lyman-FUSE mission, currently in a Phase B study under the Explorer program, will be a very powerful tool for studying the spectral region below 1200 Å, but it is not expected to fly until 1998–1999, long after the completion of the Galileo mission, and again will offer only limited opportunity for Jovian observations.

The arguments cited above lead to a single conclusion: the need for a dedicated planetary Earth-orbiting ultraviolet telescope. Only with such a facility can the dynamically varying Jovian system be adequately observed to provide support for the in situ measurements of the Galileo mission. We recommend that such a telescope be built and launched in 1996 under the Discovery program. We believe that the means are at hand to accomplish this at low cost and in a timely manner, and these are discussed in detail in the following sections. However, in the preparation of this report we have not had the resources available to conduct any in-depth studies to support our conclusions. It is important to recognize that it is necessary to begin such studies immediately if the goal of Galileo mission support is to be realized. Some detailed recommendations regarding implementation are given in the Executive Summary above.

II. SCIENTIFIC OBJECTIVES

Jupiter's magnetosphere is a dynamic object. To understand its temporal variability it is

necessary to study the coupled system of the Io torus, the magnetosphere and Jupiter's ionosphere (sketched in Figure 1). Neutral gases from Io's volcanos form an extended corona around the satellite. This material is ionized and swept up in Jupiter's strong magnetic field to form a dense plasma torus near the orbit of Io (see reviews in Dessler, 1983). The Io plasma torus is the main source of plasma for the Jovian magnetosphere: One ton per second of sulfur and oxygen ions are produced by the satellite and spread out into the large volume (1000 times that of the Sun) dominated by Jupiter's magnetic field. The magnetic field couples the charged particles in the outer magnetosphere to the planet, allowing the plasma to tap Jupiter's rotational energy, accelerating particles to high energies. As these energetic particles diffuse inwards they suffer adiabatic compression, gaining energies of up to several MeV. These particles are scattered near the outer boundary of the Io plasma torus and stream into Jupiter's upper atmosphere where they stimulate intense auroral emissions. Our understanding of this coupled, highly non-linear, system is very limited. An obvious way to study these plasma processes is to make synoptic measurements of the plasma conditions in the Io torus from the bright EUV line emissions while continuously imaging the auroral and dayglow emissions from Jupiter's atmosphere, as illustrated by the Voyager UVS scan of the Jupiter system in Figure 2 showing emissions both from the Io plasma torus and from the planet.

The Io Plasma Torus

The Io plasma torus was discovered from ground-based observations of optical emission of SII from just inside Io's orbit at $5.9 R_J$ (Kupo *et al.*, 1976; Brown, 1976). The Voyager Ultraviolet Spectrometer (UVS) later detected much stronger emissions at EUV wavelengths from the region surrounding Io's orbit (Broadfoot *et al.*, 1979). When the Voyager 1 spacecraft flew through the plasma torus the Plasma Science (PLS) instrument revealed that the torus was sharply divided at $5.7 R_J$ into a small, inner region of cold (< 1 eV) plasma consisting mainly of S^+ and O^+ ions, and a more extensive, outer region of hotter (~ 100 eV) plasma with sulfur and oxygen ions in higher ionization states (Bridge *et al.*, 1979; Bagenal, 1985; Bagenal 1989). Broadfoot *et al.* (1979) estimated that the torus emitted a total power of about 10^{12} watts. The top panel of Figure 3 shows a Voyager 1 UVS spectrum of the torus. Shemansky and colleagues have modelled such spectra to determine the ionic composition of the plasma (Shemansky, 1987), but have been hampered in these efforts by the low spectral resolution of the UVS instrument (30 \AA) and the lack of well determined collision strengths for these emissions. The lower panel of Figure 3 shows a spectrum of the torus obtained by HUT during the December 1990 Astro-1 mission that illustrates the richness of torus emissions at 3 \AA resolution (Davidsen *et al.*, 1991).

Further constraints on the composition of the torus plasma have been provided by rocket-borne spectrometers (Durrance *et al.*, 1983) and IUE (Moos *et al.*, 1985). Unfortunately, the large fields-of-view of these instruments and/or the long exposures lead to poor spatial resolution. There are many processes occurring in the torus (ionization, charge exchange, radiative cooling, thermalization, etc.) that affect the conditions of the plasma (i.e., density, temperature, composition). Thus, it is important to ascertain how the plasma evolves as it diffuses away from Io by making measurements with both high spatial and temporal resolution.

While observations by astrophysics observatories such as HST and HUT will address some of the issues of composition and spatial distribution with their high spectral resolution and high sensitivity, observing time is very limited on these astronomical facilities and to address the major issues of the operating plasma processes one needs to monitor the torus emissions with both spatial and spectral resolution. We need to measure the temporal variability of the emissions related to

the time scales of the rotational geometries. Time scales of source and loss processes leading to the emissions are also important observables. Time scales from 30 minutes to months will be useful. Short term, < 2 hours, changes of a factor of two in ansa brightness, were noted in the Voyager data. Over twelve years of IUE observations, such as those of the brightness of the SII $\lambda 1256$ emission, shown in Figure 4 (M. A. McGrath, private communication), indicate that the plasma properties vary significantly on both short and long-term time scales. However, the IUE measurements are sparse, typically only one or two per year. The Voyager UVS measurements suggest that there was a 40% increase in density and a 20% decrease in temperature of the plasma in the 6 month interval between the two Voyager encounters (Shemansky, 1987). Ground based observations of optical emissions show considerable variations, both spatially (with Jupiter's magnetic longitude) and temporally on short time scales (night to night) (Trauger, 1984; Morgan, 1985).

Dayglow and Auroral Emissions

Our knowledge of the excitation processes in the upper atmosphere of Jupiter remains incomplete, both as regards the equatorial dayglow (or "electroglow" as often referred to) and the polar aurora. Calculations of H_2 band emission spectra show large differences depending on whether the emission is produced by photoelectron excitation or by fluorescent scattering of sunlight. At a spectral resolution of only 30 Å, however, the Voyager UVS was not able to distinguish between these two excitation mechanisms. Consequently, excitation of the Jovian dayglow is still poorly understood. The lack of reliable solar data at the time of the Voyager encounter makes the problem more difficult. Although IUE operates at 10 Å resolution and can detect H_2 band emissions from Jupiter, the sensitivity is too low to extract much useful information and the emissions below 1200 Å are not measured at all. High sensitivity and spectral resolution are necessary to resolve the vibrational and rotational structure of the H_2 band emissions to settle the arguments about excitation mechanisms and reveal new information about the physical state of the Jovian upper atmosphere. These observations should be accompanied by a simultaneous monitor of the solar extreme ultraviolet flux.

Since the discovery of Jupiter's radio emission implying radiation belts in a strong magnetic field (Burke and Franklin, 1955), there has been speculation about the presence of strong polar aurora. It was first determined that the decametric emission is modulated by the orbital position of Io in 1964 (Bigg, 1964), and early indications of bright and variable HI Lyman- α emission were recorded in a rocket experiment by Rottman *et al.* (1973) and in Copernicus observations by Atreya *et al.* (1977). The first spatially resolved observations of localized FUV polar auroral emissions came from the Voyager 1 UVS (Broadfoot *et al.*, 1979) and, as illustrated in Figure 5, by IUE (Clarke *et al.*, 1980; Skinner *et al.*, 1984; Livengood and Moos, 1991). The nature of the spectra and the location of the emission should reveal the characteristics of the particles that are precipitating from the magnetosphere into the upper atmosphere and stimulating the emission (Herbert *et al.*, 1987; Gladstone and Skinner, 1989). IUE has the advantage that one can make repeated measurements over a much longer period; Skinner *et al.* (1984) and Livengood and Moos (1991) have been able to determine the longitudinal dependence and temporal variability of Jupiter's auroral emissions over 10 years. They observed variations in emission intensity of as much as 50% over time scales as short as a rotation period. Unfortunately, the spatial resolution of IUE is insufficient to resolve the auroral oval. Pioneer and Voyager instruments measured a sharp drop in energetic particle fluxes between $L \sim 12$ and Io's orbit. One expects that mapping these field lines to the atmosphere of Jupiter, using the best-available magnetic field model, should give the location on the planet. However, Voyager UVS measurements of auroral emissions are not consistent with an auroral oval

defined by the magnetic field (Broadfoot *et al.*, 1981). Figure 6 shows the auroral oval to be centered on the rotation pole rather than the magnetic pole. This implies a very strong variation with magnetic longitude which is very hard to understand because the rapid drift motions of these energetic particles would suggest that Jupiter's radiation belts should be highly rotationally symmetric. Clearly, high resolution images of the auroral oval would resolve this important issue.

More recently, the H Lyman- α line profile from the auroral atmosphere has been resolved using the high dispersion spectrograph on IUE (Clarke, Trauger, and Waite 1989). The line profile from bright aurora shows no evidence for fast precipitating protons ($E > 100$ eV), but shows substantial evidence for low energy motions of the emitting atoms (10–20 eV) with a direction mainly up out of the atmosphere. This may be explained by ionospheric particles accelerated in situ by field-aligned potentials, and in this sense the Doppler-shifted emission is a tracer of the ionospheric currents which link the auroral ionosphere with either the torus or magnetospheric convection patterns.

Other Solar System Targets

While bearing similarities with Jupiter, the magnetosphere/atmosphere coupling of the other giant planets, Saturn, Uranus and Neptune, are each unique. Observations of ultraviolet emissions from these planets will address important scientific questions about the particle populations in the magnetosphere and their deposition into the planet's atmosphere. Regarding the terrestrial planets, measurements of emissions from the constituents of the atmospheres of Mars and Venus reveal the atmospheric structure, chemical processes and solar wind interaction of these planets. The UV spectra of cometary comae have also proven to be extremely rich in elucidating both the composition and evolution of the gaseous component of the coma and, by inference, the composition of the ices which comprise the bulk of the surface material of the small, solid nucleus of the comet. Such observations will be possible after the completion of the Galileo support phase of this mission, and will be solicited from members of the community through a guest observer program.

III. APPROACH

We propose a low cost method of maximizing the mission performance while keeping the risk of catastrophic failure very low. We are prepared to risk less than optimal return of science data in exchange for a dynamic, short term program. The essence of our approach is to use a very small team of experienced, dedicated people and to rely on their judgment in the decision processes. Low cost can be achieved by having good definition at the start, minimizing the design/fabrication/test schedule and having the same people working continuously from start to finish.

Integrated Team

In order to perform this mission at the lowest possible cost it is essential to minimize the interface complexity in all areas including hardware, software, mission operations and project management. Experience has shown that, even with a simple interface, excessive conservatism is generated on either side of the interface and an upward cost spiral results that is difficult to control. Our solution to this problem is to have a very compact team, led by a Principal Investigator, having responsibility for all the hardware, software and mission operations. It is important that this team write a completely defined proposal prior to starting hardware activities (Phase A). The team should consist of a small number of scientists who have major roles to perform during the project

and engineers and technicians to design and build the hardware. This highly integrated team would produce the fully integrated hardware including all the instrument and spacecraft components. All the work should be concentrated at one location to minimize communications problems. To achieve this team, it is necessary to combine the talents of scientists and engineers together in a positive working atmosphere.

Organizational Structure

We envision a very small administrative project office at a NASA center similar to that used for the Solar Mesosphere Explorer at JPL. The integrated team would consist of a small number (4-6) of planetary scientists, together with engineers from a highly qualified company. The team leader would be the Principal Investigator and would be supported by the contractual program manager to ensure that the compact team structure works. The scientists would have well defined duties to perform and would be integrated into the engineering team to get the job done. Other scientists would participate in the mission at a low level during the hardware phase and then more directly during the data gathering and analysis phase.

Integrated Science Instrument/Spacecraft

As we do not propose an array of science instruments, it is not necessary to have a spacecraft bus. We propose to fly an instrument surrounded by the minimum spacecraft components necessary to get the science data. This approach has the effect of reducing cost without increasing risk significantly. The number of interfaces is reduced so reducing the manpower necessary to service those interfaces. As an example, the use of the scientific instrument for fine-pointing control of the spacecraft reduces the need for external components to perform the Fine Error Sensor function. In this way, the weight of the spacecraft is reduced, permitting more launch margins. Optimization of spacecraft sub-system performance to suit instrument requirements results in weight, power and cost savings. The system can be optimized by viewing the entire instrument/spacecraft as a single entity.

Risk Management

It may be thought that the biggest risk in this organizational structure is in trusting team members not to make errors. We believe that this perception is not valid and regard this trust as being the key source of success in implementing the Discovery mission. The biggest risk is in changes in scope. Changes cover all aspects of the program from requirements to the funding profile. We recommend that the UV Discovery mission be activated and follow an implementation plan as proposed with no top-level changes. It is important to give individuals plenty of freedom to work in their area of responsibility but to restrict any changes that ripple over into other people's domain.

Schedule

As the basic concept of the Discovery program is to obtain results in as short a time as practical at the lowest cost, we plan to use a radically different approach to normal NASA programs that has been proven on some DoD programs. The concept is to use a "finish ahead of schedule" philosophy

rather than a "critical path planning schedule". In other words, have sub-systems waiting for integration instead of having everything come together at the same date. The major advantage of this approach is that the team can concentrate on the difficult problems knowing that everything else is ready. Sufficient funding must be available early in the program to make this happen. The pace of a program of this nature is such that some parallel procurements will be made to guarantee early sub-system completion. The price of such redundancy is low when compared with the cost of solving problems later in the program.

We propose to complete the UV Discovery hardware phase from the initiation of funding to launch in less than three years. Such a schedule requires that adequate studies be performed prior to project inception to define all of the critical design and procurement areas so that the Preliminary Design Review can be held within a few months of the start. Fabrication requires ~ 18 months followed by 12 months of alignment, calibration and system testing. Milestones associated with a possible schedule are given in Table 5 of the Ball Aerospace appendix.

Incentives to Meet Goals

The major incentives to maintain cost, schedule and performance are enthusiasm, recognition of each members contribution, responsibility and adequate financial resources. Enthusiasm can only be maintained for short time periods, hence our interest in short schedule and early delivery of sub-systems. Individuals and organizations must have high visibility in the program and to be made to feel responsible for their share of the project.

IV. FEASIBILITY

Description of Payload

The basic concept is that the scientific instrument and the spacecraft, with all of its supporting functions, is considered to be a single unit which is to be designed and built by a small, highly integrated team. That this is feasible can be demonstrated by considering the nature of the instrumentation required to achieve the scientific goals stated above. The scientific instrument comprises a telescope, usually cylindrical in the shape of its envelope, which includes the necessary baffling, and one or two focal plane devices, which for our purposes might consist of an imaging spectrograph and an EUV camera. The focal plane instruments are located behind the telescope and can usually be accommodated in an extension of the cylindrical volume. Typically, the length of the cylinder will be allocated about 2/3 for the telescope and 1/3 for the focal plane instruments. For our goals, a telescope diameter of from 50 to 90 cm suffices, although specific instrument parameters await a more detailed study.

The remainder of the spacecraft, the traditional "bus", is placed around the back 1/3 of the cylinder and integrates completely with the optical bench assembly of the focal plane instruments. Several of its components are common, such as the computer used for command and data handling and the slit-jaw camera which is used for acquisition and tracking. Power is obtained from solar cells which are wrapped around the outside structure of the cylinder. While this is not as efficient as a separate planar array, it allows for arbitrary position angle alignment of the focal plane slit on the sky, which is vital to the Jovian objectives given above.

Since the primary objectives require spectroscopy and imaging below 1200 Å (the extreme ultraviolet, or EUV), and our goal is to achieve these objectives with normal incidence optics,

multiple focal plane instruments requiring additional transfer optics components are not feasible. Instead, a possible approach is to split the focal plane, using one half for direct EUV imaging and the other half for long-slit spectroscopy. This latter half would also act as the slit-jaw camera for visible imaging of the Jovian system for ground control of the acquisition and tracking of the telescope. With this approach, imaging and spectroscopy are achieved simultaneously. Although several means of achieving the split field are available, further work is needed in finding the optimal design. Note that while the spectroscopy provides approximately 1 arc-second spatial resolution along the slit, the EUV camera is simply a broad-band imager with a band-pass of approximately 600-1100 Å.

Detailed consideration of the focal plane instrumentation is omitted here. There are several means of achieving the goals of spectral and spatial resolution. These have been developed in various recent proposals or have been demonstrated in recent sounding rocket and Space Shuttle flights. There also have been significant advances in ultraviolet technology (e.g., silicon carbide coatings; ion-etched holographic diffraction gratings; multi-element photon counting array detectors), many as a result of instrument development for other NASA programs, which are applicable to the proposed payload. The choice of focal plane instrumentation, including the important consideration of science data return vs. cost, is one of the major activities to be performed during the Phase A study.

The attached appendix, prepared by Ball Aerospace under a study contract, presents a description of a "strawman" payload consonant with the ideas stated above.

Orbit considerations

Experience with ultraviolet observatories, such as Copernicus, HST, and Astro-1, have shown the disadvantage of low-Earth orbit (LEO). These disadvantages include encounters with the South Atlantic Anomaly, stray UV from the Earth's airglow and geocorona, interruptions by Earth occultation, and the difficulty of operation with simple fixed ground stations. High-Earth orbit (HEO), such as used by IUE, is much more favorable and provides near-continuous observing time, direct communication, and freedom from the Earth's airglow with the exception of geocoronal Lyman- α . HEO suffers from occasional infringement of the trapped radiation belts plus the mass penalty of lifting the telescope into a high altitude, nearly circular orbit. Highly elliptical orbits in the HEO category suffer from low perigee heights which reintroduce the radiation belts and near-Earth problems.

An attractive option is an orbit near a libration (Lagrangian) point (LP) of the Earth-Sun system. The L_1 point is 1.5×10^6 km sunward and L_2 is 1.5×10^6 km anti-sunward, and both lie on the Earth-Sun line. These points share all the positive features of HEO but also offer freedom from near-Earth airglow and geocorona contamination. In addition, the unchanging conditions provide an excellent environment for a spacecraft that wants to minimize disturbances. There are no aerodynamic torques, no gravity gradient forces, no varying illumination effects from the Earth, and no radiation belts.

The energetics of HEO and LP are surprising. From low-Earth orbit it takes a Δv of 4 km s⁻¹ to achieve HEO. To reach the LP requires a total Δv of about 3 km s⁻¹. This means that a libration point observatory can be significantly larger or cheaper than a HEO observatory. As an LP observatory is at a significantly larger distance from the Earth, data rates will be lower, but for this mission, this consideration does not appear to be a limiting factor. The mass/energy tradeoffs are almost even between LP and a highly elliptical HEO, but, again, the latter suffers from periodic

interruptions and regular exposure to the radiation belts.

A 24 hour HEO has the advantage that a single ground station can serve as the control and data collection facility. An LP observatory is visible from a single station for 12 hours per day on average. If true 24 hour/day realtime is needed, low elliptical HEO is the only choice. If 12 hour/day of realtime contact is acceptable, the the LP is a candidate. Moreover, the Earth is available as a solar system target from the LP. At the L_1 (sunward) point the Earth subtends 0.5 degrees and will be almost fully illuminated continuously. The large halo orbit of the spacecraft around the L_1 point allows Earth observations of up to 20 to 30 degrees from the Earth-Sun line in the E-W ecliptic direction. In the N-S ecliptic direction the spacecraft will move a maximum of about 10 degrees above or below the Earth-Sun line. At the L_2 point (anti-sunward) the Earth is largely dark, but will appear as a thin crescent with a maximum phase angle of about the 20 to 30 degrees mentioned before. When the spacecraft is above the ecliptic plane by 10 degrees and the Earth's geometry allows the maximum 23.5 degrees plus 11.7 degrees tilt of the magnetic axis, most of the northern auroral region will be visible on the dark side of the planet.

Also, we note that only a small amount of onboard propulsion is needed for orbit maintenance in HEO and at the LP.

Operational Approach

The combination of a high-Earth orbit and a single target for the prime mission (or a few, fairly bright targets after the prime mission is completed) requires a relatively small ground support operation that could be carried out in a university environment. The functions would include monitoring of the spacecraft health, target tracking, and data acquisition and archiving, and will be done directly under the supervision of the science team members who will be responsible for the reduction of the data into physically useful form and disseminated to the user community.

V. COST ESTIMATE

Without the benefit of detailed technical studies, it is not possible to derive an accurate cost estimate for the UV Discovery mission described above. However, it is possible to draw on experience with existing spacecraft and scientific instrument hardware and project personnel requirements to arrive at an order of magnitude estimate of the cost of a mission following the guidelines that we have established. This estimate, in current year dollars, is \$50-70 M, exclusive of launch vehicle cost. We regard it as a major objective of a follow-on study, to determine this number in a reliable, realistic manner.

VI. PROPOSED NEAR-TERM ACTIVITIES

We propose to continue the activities of the UV sub-group of the Discovery Science Working Group to complete, in some detail, the study described in the present report. Such a study would encompass most of the activities associated with a traditional Phase A study, and would be done during the first half of FY1992 for an estimated cost of \$110,000. The format of the study would consist of two three-day workshops involving both the scientific study team and the engineering support study team. The workshops will be separated by 2 to 3 months during which problems identified at the first workshop will be addressed. The second workshop will produce a final report which will include a detailed cost estimate for the mission.

The study tasks will be addressed in a priority ordered manner to ensure that the available resources are allocated where they are needed most. As a result of the present study we have identified the following as some of the leading tasks:

Science Objectives: Determine the instrument parameters required to achieve the scientific goals outlined in Section II above. Such parameters include spectral range and resolution, spatial resolution, sensitivity, pixel size, tracking stability, data rate, etc.

Optical Design: The telescope aperture and focal length as well as the requirements of the imaging spectrograph must be constrained to the available volume and weight of the launch vehicle and must simultaneously satisfy the science objectives of the mission.

Mirror Constraints: Mirror designs to achieve low weight and dimensional stability are to be evaluated. The procurement of the telescope mirrors (and the spectrograph grating) is a long lead-time activity and ultimate schedule driver.

Flight Computer and Electronics, Data Handling: The possibility of a single flight computer for both the spacecraft control and the scientific instruments is to be evaluated.

Structural and Thermal Design: Once the tasks listed above are complete the structural and power needs can be determined as well as the total mass of the spacecraft.

Cost and Schedule.

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FIGURE CAPTIONS

- Figure 1. Sketch of the Jovian system showing the coupling between the Io torus, the magnetosphere and the upper atmosphere of Jupiter.
- Figure 2. The Voyager UVS scan of the Jupiter system showing both emissions from the Io plasma torus and the planet.
- Figure 3. (top) The EUV spectrum of the Io plasma torus obtained by the Voyager Ultraviolet Spectrometer (Broadfoot *et al.*, 1979). (bottom) Portion of Io torus spectrum obtained by HUT during the Astro-1 mission in December 1990 (Davidsen *et al.*, 1991).
- Figure 4. Brightness of the SII $\lambda 1256$ emission from the Io torus as recorded by IUE over a twelve year period.
- Figure 5. Longitudinal variation of Jovian auroral emission as seen by IUE.
- Figure 6. Comparison of the Voyager UVS map of Jupiter's auroral zone and the magnetic field lines connected to the Io torus.

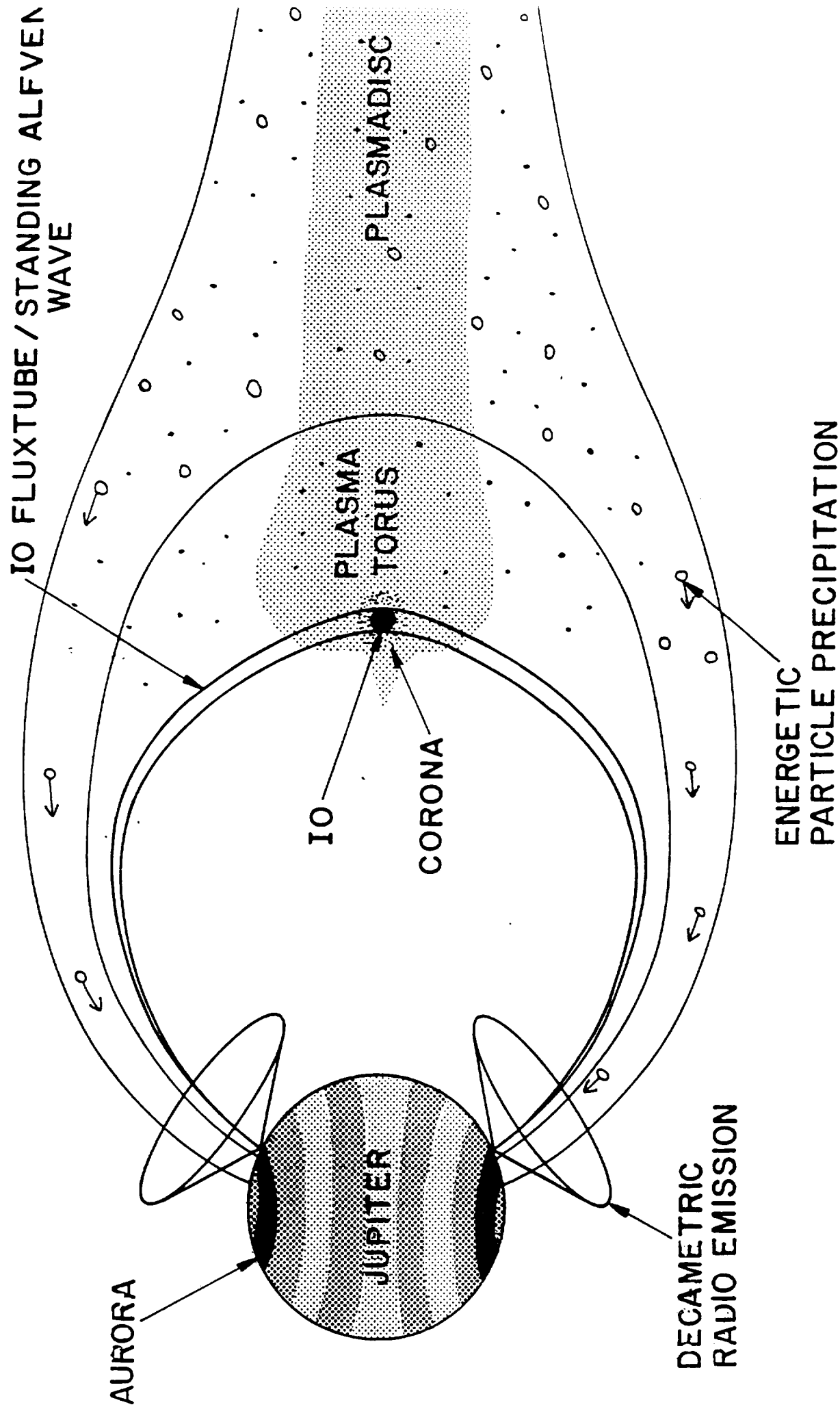


Fig. 1

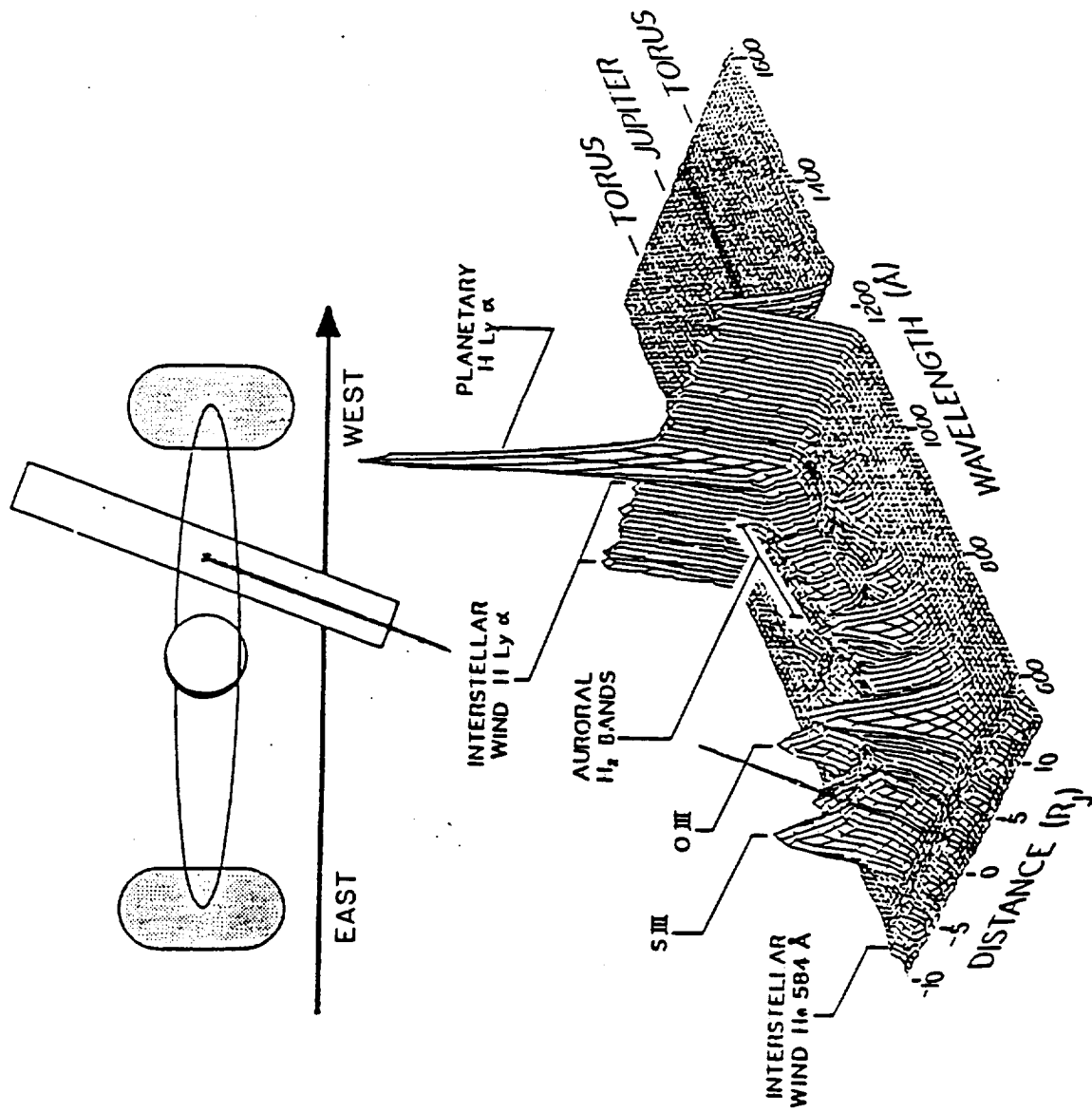


Fig. 2. UVS System Scan

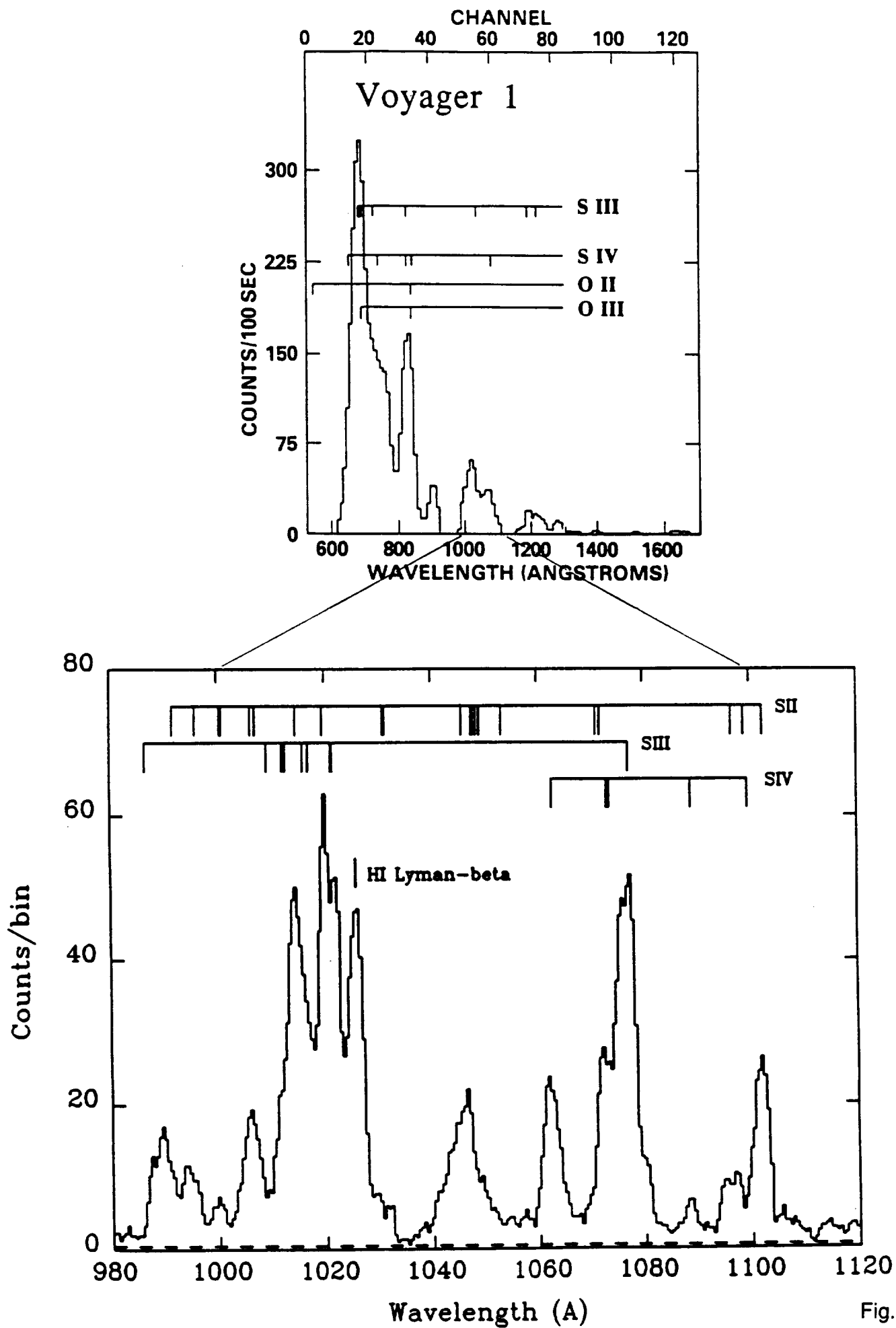


Fig. 3

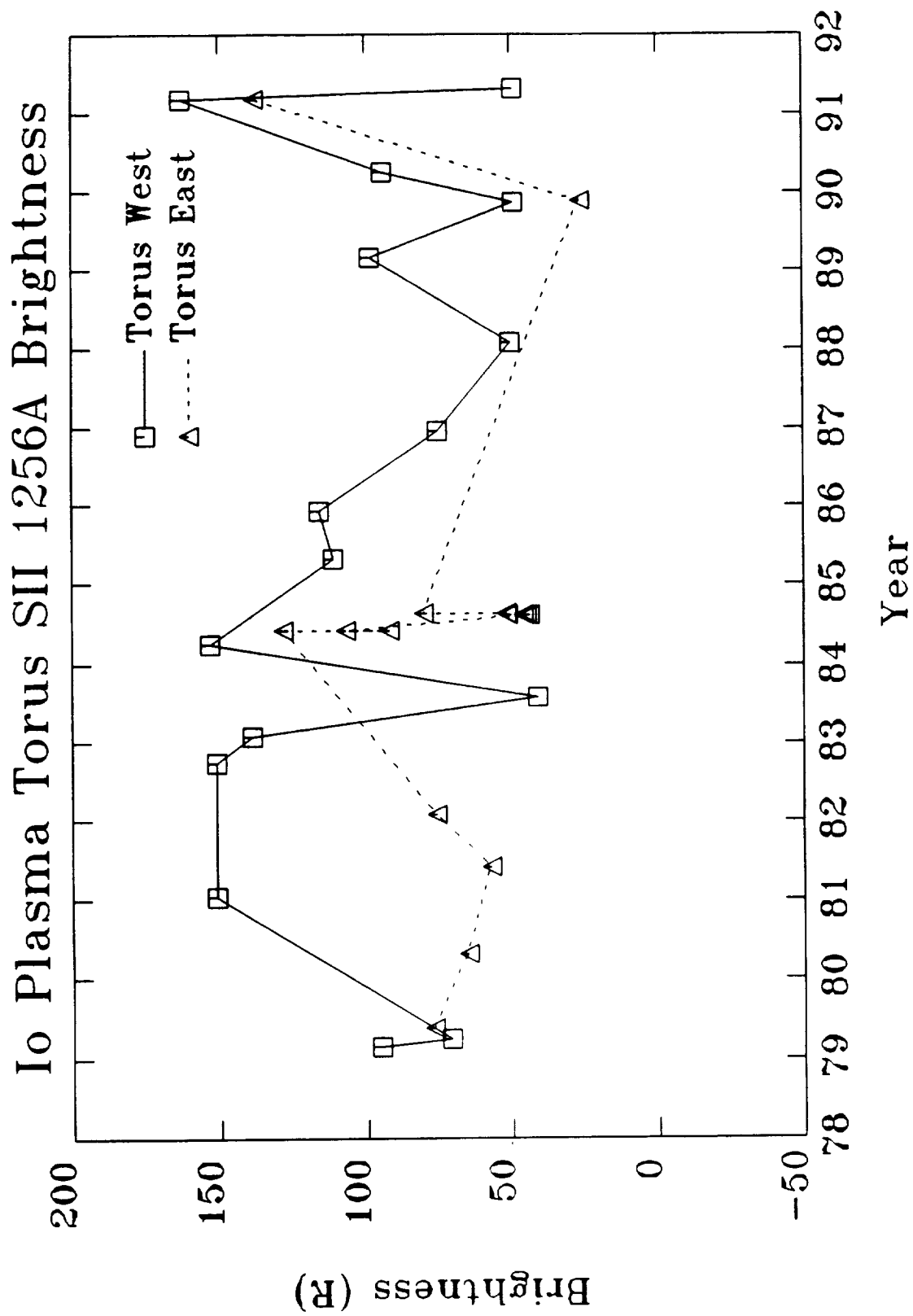
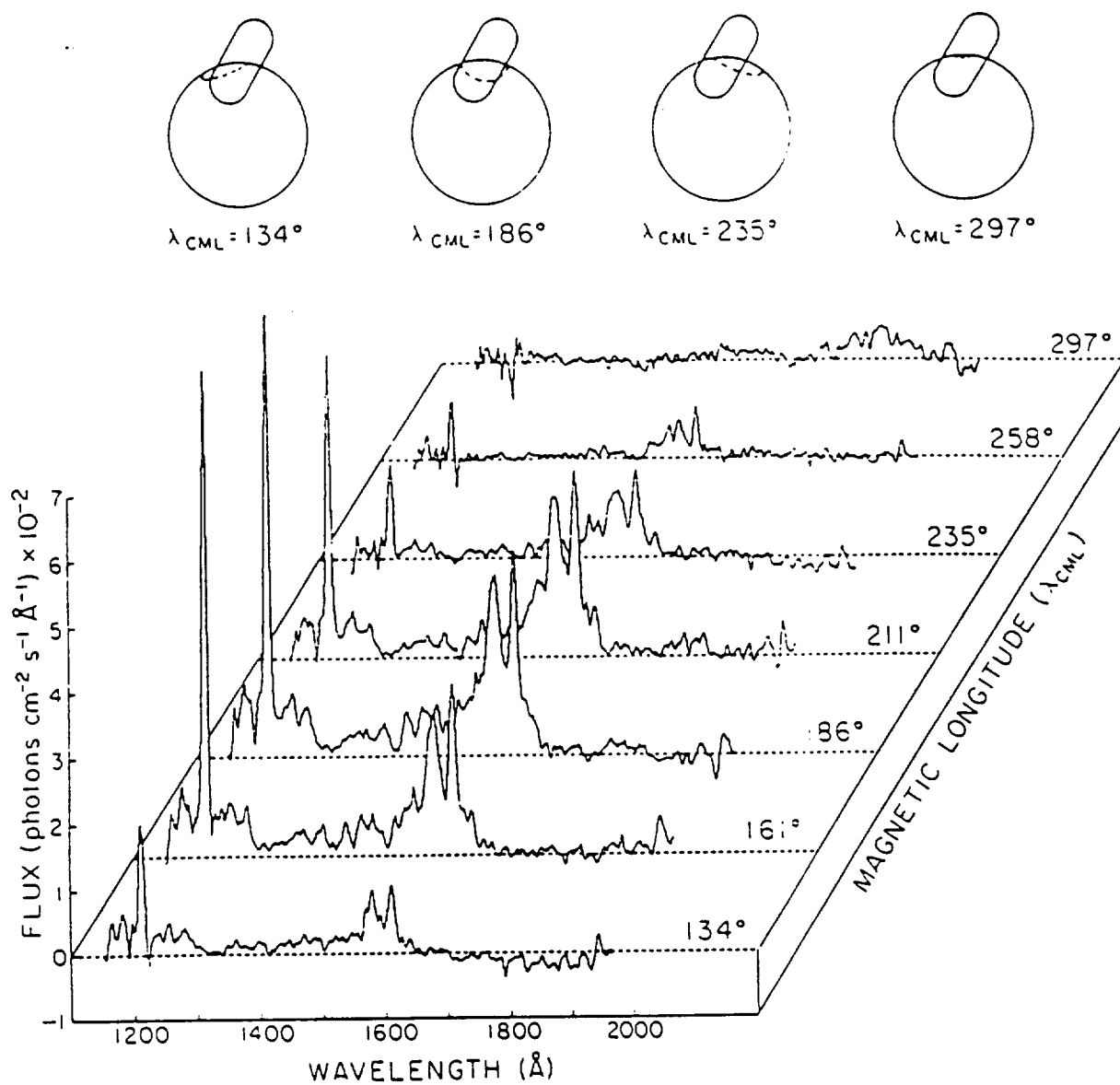


Fig. 4

JOVIAN AURORAL EMISSION



Skinner et al. (1984)

Fig. 5.

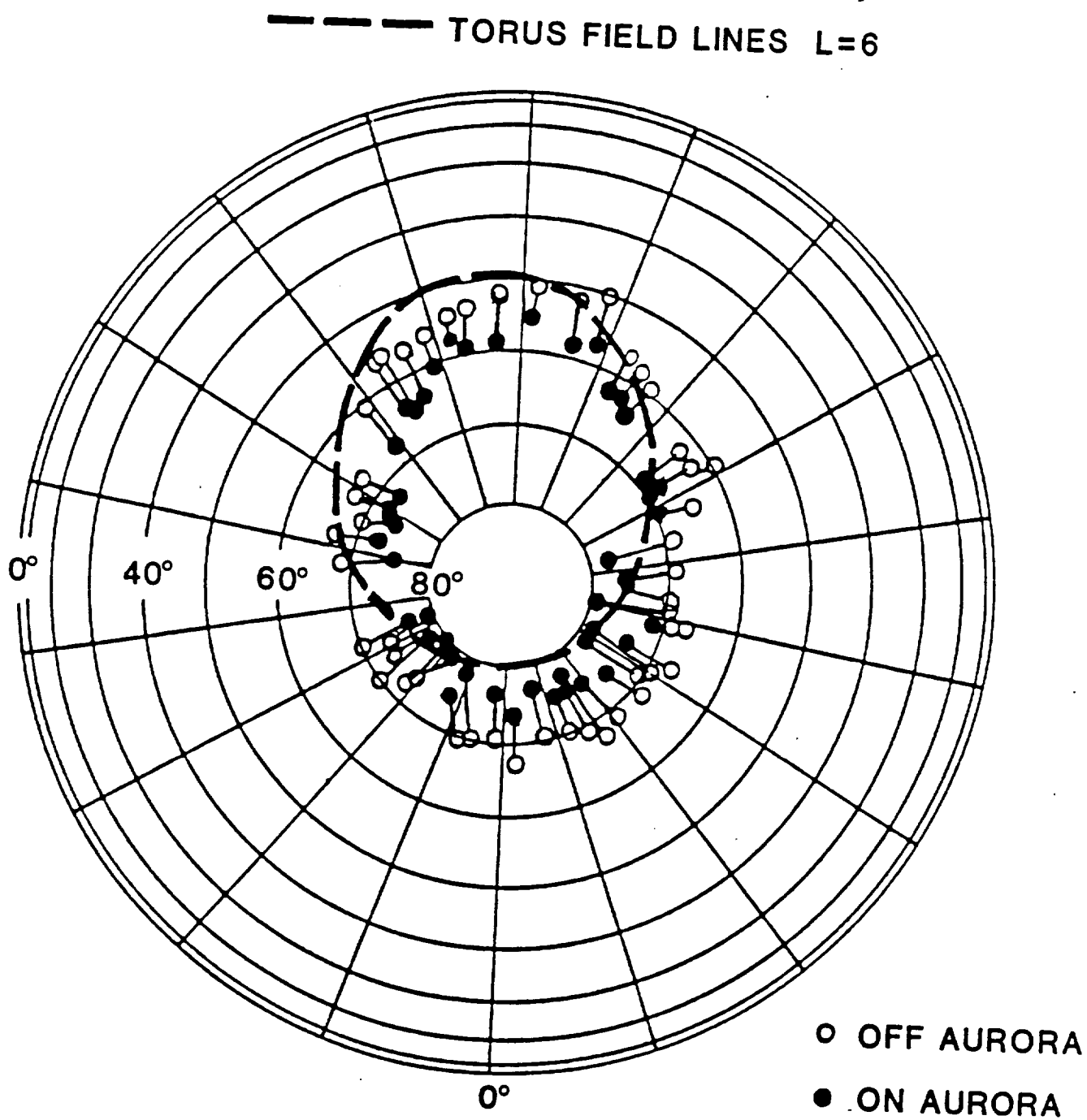


Fig. 6

